AMERICAN SOCIETY OF CIVIL ENGINEERS.

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CLXIII.

(Vol. VII.—July, 1878.)

ON A NEWLY DISCOVERED RELATION BETWEEN THE TENACITY OF METALS AND THEIR RESISTANCE TO TORSION.

By ROBERT H. THURSTON, Member of the Society.

READ AT THE TENTH ANNUAL CONVENTION, JUNE, 18TH, 1878.

The determination of the nature of the relation which exists between the resistances of the metals to torsion and to tension, is a matter of both interest and importance to the engineer, and has also great importance as bearing upon the theory of the resistance of materials. It has always been assumed by the engineer and the physicist that there exists a constant relation between these two forms of molecular resistance, and that the resistance of a homogeneous material to rupture or to distortion by tension could be predicated upon the results of experiments determining its resistance to torsion, and that the modulus, or measure, of the latter could be derived from that obtained for the former by multiplying by a constant co-efficient, which has a certain value ascertainable by experiment for every case,

Were the generally accepted theory of torsion correct, which assumes that form of resistance to distortion to be a case of shearing, the above assumption would hold, and the use of a constant factor would be permissible, and would afford perfectly accurate results. This theory makes the resistance to torsion to vary directly as the distance of any given line of particles from the axis, and as the angle of torsion, and makes the molecular resistance to disturbance to be equal to that due to molecular change produced by tension. A study of the strain-diagrams obtained by torsional tests shows that the assumption of resistance varying directly as the angle is correct up to the limit of elasticity. Were the theory correct throughout the whole progress of the distortion, the equation given by the text-book would be the equation of the curve of the autographic strain-diagram.* This is not the case, however, as can as can readily be shown by plotting the equation and comparing with the strain-diagram of any given material.† The equation represents the strain-diagram of no material known in the arts where the curre passes the limit of elasticity.

The discussion of the real form of the true equation of resistance by torsion must be reserved for another paper.

The writer has found, however, that the relation here referred to is a very simple, as well as a constant one, and has empirically determined its character.

It has been customary to determine from autographic strain-diagrams of the metals produced by the torsion machine approximate values for tension by simply assuming the vertical scale of the diagram produced by test-pieces of standard form and size to have a value of 25 000 pounds per square inch for each vertical inch, representing, as is usual, 100 foot-pounds of resistance to torsion. The ratio of 250 to one was originally determined empirically by experiments upon wrought-iron, and was found, as usually applied, to be quite correct. More recent comparisons with cast-iron proved the ratio for that metal to be less; but this was at first attributed to the almost absolute impossibility of securing a perfectly fair pull in testing in the common testing-machine, as well as to the theoretical difference between the brittle and the ductile material. It was not then suspected that the universally accepted relation of the two forms of resistance was incorrect.

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* See "Resistance of Materials." By De Volson Wood, N. Y. 1875. P. 208.

† See Wood's Resistance of Materials, p. 263, and Frontispiece.

Still later experiments, made by Dr. Dudley, with the autographic machine of the Pennsylvania Railroad Company, at Altoona, and comparisons with direct experiments in tension, indicated a scale differing about five per cent. from that which the writer had been accustomed to use. The metals experimented with were all "mild" steels, such as are used in the manufacture of rails. The writer had still further observed that the metals which were marked by an extraordinary ductility exhibited an apparent excess of strength when tested by torsion. These differences, except in the case of non-ductile materials, as cast-iron, were not sufficiently marked, so far as the comparisons thus made went, to seriously affect the value of determinations which were obtained on the assumption first made. They finally became sufficiently obvious, however, to lead the writer to make the attempt to determine by direct comparison the true relation existing between the two kinds of resistance to distortion.

In order to secure the widest possible range of comparison, it was concluded to examine this relation, and to determine the value of the ratio of the scales for tension and for torsion for the widest range of grade of steel that could be found upon the records of the Mechanical Laboratory of the Stevens Institute of Technology. The records of test in both tension and torsion were obtained for a very large number of steels of all grades, from high tool steels containing one per cent. of carbon and upwards, to the softest "homogeneous" metals, in which the percentage of carbon fell to one-fourth or less. The values of the required ratios were determined within this range for about thirty steels by Mr. W. R. Baird. As the writer had been led to suspect that the value was variable, and that it was some constant function of the total angle of torsion,* the values obtained were laid down on a sheet of profile paper, on which the ordinates represented the numerical value of the ratio obtained by the division of the tensile resistance in pounds per square inch of original section to the resistance of the standard test-piece in foot-pounds of torsional moment, while the abscissas were proportional to the total angle of torsion at the moment when the test-piece exhibited the maximum resistance as measured upon the autographic strain-diagram. The accompanying plate (XXII.) is a copy of that prepared as described. It is seen that the values thus recorded have a regularity which is probably quite as great as should be expected when the irregu-

* See Wood's Resistance of Materials, p. 204.

larity of structure of samples, due to causes apart from their composition, is considered. No pair of samples compared are ever of *precisely* the same character, either physically or chemically, and the ratio for samples cut from the same bar will therefore sometimes exhibit some variation.

Drawing a mean line through the group of determinations, it was found that it was approximately straight, *A B*, although probably really a parabola, *C D*, having one of its asymptotes not far from the height 200, but somewhat below it.

No determinations have yet been made for materials having maximum resistance at angles of torsion exceeding 300 degrees, and the law cannot therefore be traced beyond that point. As all the materials in general use in engineering attain a maximum within that limit, the law determined here is sufficient for all practical, if not also for all scientific purposes. Since the ratio varies, as is seen in the figure, inversely as the angle of torsion, it follows that so long as the molecular resistance to distorsion remains constant, the ordinate of the strain-diagram must diminish with increase of the angle of torsion. Uniform molecular resistance would thus be exhibited by a gradually falling line.

The equation for the ratio here studied, as determined from Mr. Baird's comparisons, is : $R = 300 - \frac{\Theta}{3}$, and $T = T^1 (300 - \frac{\Theta}{3})$; in which R represents the ratio and T and T^1, respectively, are the tenacity in pounds per square inch, and the torsional resistance of the standard test-piece* in foot-pounds of moment, and Θ the angle of torsion for maximum ordinate. This gives, for the standard diagram, a vertical scale, per inch of height, of $(30000 - 33{\cdot}33\ \Theta) = T$.

At the angle 150 deg., the scale, 25,000 pounds, is closely accurate. For nice determinations it is better to take a value two per cent. lower—24,500. It thus happens that determinations hitherto made on wrought-iron have been fairly, as they are now perfectly, comparable with those made by the old method.

It singularly happens that the commonly accepted theory predicts values of this relation which are not only incorrect but which are, for different cases, precisely the opposite in their mutual relations to those here given by experiment.

The value predicted by common theory is, for brittle substances, 250, and for ductile materials 188, the non-ductile substances giving the higher

* The standard test-piece for the autographic machine is 0.625 inch in diameter, and 1 inch long.

value. But we have now learned that the latter class of materials, as represented by cast-iron give a value which is nearly, but perhaps slightly higher than that theoretically obtained for ductile metals: while the ductile class, as represented by the steels, give a value which is variable, but which, at the low angles of torsion at which they are comparable with the brittle metals, somewhat exceeds the values theoretically given for the latter, thus inverting the relation of these values as predicted by accepted theory.

The autographic strain-diagrams of which copies have been given in earlier papers by the writer, have usually had a vertical scale of 100 foot-pounds to the inch of ordinate. This is the scale now always adopted. For such diagrams, therefore, the vertical scale for tension will be, approximately:

For cast-iron.....	20,000	pounds	per inch.
For wrought-iron.....	25,000	"	" at 150 deg.
For steels (rail).....	25,000	"	" "
" " (tool).....	28,000	"	" 60

For other angles, the scale may be calculated as above where great nicety is demanded. Where the maximum ordinate remains constant in value, the upper part of the diagram becoming a horizontal line, the angle taken for measurement should evidently be that at which the ordinate first attains its maximum value.

The facts here detailed are, perhaps, insufficient to determine whether the singular and unanticipated relation revealed by them is due to some peculiarity in the method of action of torsional strain, or to the possible fact that those molecular modifications of cohesive force which produce a variation in the value of resistance to tensile stress, may produce variation in resistance to torsional stress in a different ratio. The indications seem to the writer to point toward the latter conclusion, and experiments upon the several grades of steel with a view to determining the effect of various chemical elements upon their strength and ductility, afford some ground for a similar deduction. Until this question is fully decided, the relation here revealed cannot be made a basis for a new and correct mathematical theory of torsion. It is hoped that, ere long, however, the needed facts may be supplied and that theory established.

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CLXIV.

(Vol. VII.—July, 1878.)

OBSERVATIONS ON THE STRESSES DEVELOPED IN METALLIC BARS BY APPLIED FORCES.

By THEODORE COOPER, C. E., Member of the Society.

Read at a Meeting of the Society, July 17th, 1878.

While testing some steel links at the Keystone Bridge Works on June 26th, the writer accidentally noticed upon the link heads peculiar markings due to the cracking of the scale while under the test. A close examination of the lines convinced him that they were indications of the mode of transfer of the stresses about the pin-hole and throughout the head of the bars.

Careful sketches were made of all the lines upon the several sides of the heads.

Believing that the observations were entirely new and worthy of careful examination and study by those interested in physical phenomena, the writer has recorded all the data, and now desires to report the same.

§ 1. The links tested at this time were two in number. The material was steel, of the kind known as Hay's steel; having been made by a special process at the Edgar Thompson Steel Works as an experiment. By analysis of borings taken from the links, the carbon is 0.33%.

The links were forged from solid billets by Messrs. Hussey, Howe & Co., and sent to the above works for testing. The links were neither welded nor upset.

Test of No. 1 *Link.*—Section of body of bar, 3″×1″.1. Section of heads 7¾″×1″.2. Shape of heads as shown upon drawings.

Length from centre to centre of pin-holes, 2′ 9″—diameter of pin-holes, 3″—difference between pin and pin-hole, $\frac{1}{32}$″.

This bar was unannealed, being left to cool in same condition as when last worked under the hammer. Up to 40,000 pounds per □″ no permanent set was visible. At 43 350 pounds per □″, the scale commenced to fall off from the body of the bar. At 64 000 pounds per □″ (on body of bar) a bolt in the testing machine broke and the bar was removed. This accident brought to view the scale markings before mentioned.

Figs. 1 and 2 show the lines visible at this time upon the opposite sides of the head called *A* (for distinction); Figs. 3 and 4, those for head *B*. The pin-holes had elongated $\frac{3}{32}$ of an inch; the bar 5 per cent.

After recording the lines and other data, the bar was again placed in the machine and broken. It cracked outwards from the pin-hole in head *B* at a strain in body of the bar of 71 700 pounds per □″, showing a hard bright fracture.

From the appearance of the scale on this head and the fracture, the bar at this end had been overheated. The breaking through the pin-hole was, however, partly due to the distortion of the pin-hole under first strain and a want of care in securing a careful bearing upon the pin when replaced.

Figs. 5 and 6 show the lines visible upon head A after rupture of the bar. But few lines were visible upon head *B*, and these very imperfectly marked. The scale upon this head was coarse and uneven, and appears unsuitable for recording these lines.

Test of No. 2 *Link.*—This was a duplicate of No. 1, being of same dimensions and material, differing only in having been annealed by heating evenly to a red heat and then allowed to cool uniformly in the open air. No permanent set visible at 40 000 pounds per □″. After the

discovery of the lines upon No. 1 link, greater care was taken to observe the first appearance of them upon this second link. The result was exceedingly satisfactory and interesting, and the writer believes the key by which to interpret these lines was discovered.

The scale commenced falling at 45 700 pounds per □″. At 47 000 pounds per □″ distinct lines appeared upon a portion of the body of the bar. The strain was relaxed and the bar carefully examined. For about two inches the bar was distinctly marked with diagonal cross lines at 45° (as well as could be estimated by the eye) to the line of the bar. These markings were visible upon all four sides of the bar; the number of the lines being about 5 to the inch measured perpendicularly to the lines.

Fig. 7 represents the condition of the lines at this time.

On applying an increased strain, new lines appeared to the right and left of those which first appeared. As the new lines appeared, the old ones disappeared by the subdivision and disappearance of the scale.

The first appearance of the lines upon the heads of the link could not be observed from their position in the machine.

At 72 000 pounds per □″ the bar broke in the body at the point where the cross lines first appeared.

Stretch of bar =13 per cent.

Reduced section at points of rupture $= 2\frac{1}{4}'' \times \frac{2}{3}\frac{1}{2}'' = 33$ per cent. Upon both heads of this link, lines almost identical with those of Figs. 5 and 6, were visible, but not so distinctly marked.

§ 2. On considering these lines, the writer recalled to mind, having seen at a previous time eccentric lines about holes punched in steel plates, which he at that time thought were due to vibrations of the plate from the effects of the blow of the punch. These he has since re-obtained, and gives a representation of one in Fig. 8.

He has made, since the first discovery of those scale markings, many experiments upon specimens of steel and iron in order to investigate them. In iron he has not succeeded very well as the scale appears unsuitable. Various resinous coatings have been tried, but with only partial success. Upon one iron link (full sized bridge link, $5'' \times 1''$) he obtained a few lines at the four diagonal points of the link-head, corresponding exactly to those shown upon the steel links. This is his only success as yet upon iron. Upon steel specimens he has obtained quite a number. It appears to require a scale of a peculiar character; many

samples failing entirely, until the right scale is obtained. To avoid too many illustrations, a few sample sketches are given.

Fig. 9 is a compilation of lines obtained upon a number of grooved specimens of the form shown. Over a dozen specimens have been tested. No specimen showed all of these lines, but each one showed a portion. By combining such as were visible on the different ones, we have obtained Fig. 9. Those in the vicinity of the groove appear first at about 40 000 pounds per □″ on the grooved section ; those about the pin-holes only at or near the rupturing point, which was usually from 89 000 to 95 000 pounds per □″ on grooved section.

Figs. 10 and 11 show a sample of lines produced by the shearing of a steel plate under a shear, one being from the top of the plate and the other from the bottom.

These lines are less definite in character than those from direct strains, undoubtedly due to the fact that the strains are very indefinite, being partly a shearing and partly a bending stress.

§ 3. It will be noticed from all these sketches that the lines appear in both directions, and cross one another at right angles. They are also at 45° to the line of applied stress in Figs. 7 and 8, where the *line of stress* is well determined. In the first case, being parallel to the direction of the bar, and in the second, on radial lines.

The writer thinks that it is also true that the direction of the transferred stresses upon the link-heads and in the grooved specimens are also at 45° to the markings. His reasons for so thinking will be expressed further on, as he desires to present his facts independent of all theories, and allow readers to take each, independent of the other, if they so desire.

§ 4. M. Tresca, before the Science Conference at South Kensington Museum, in 1876, states that while forging, on a large scale, some iridescent platinum, he noticed that "when the bar of platinum, at the moment of forging, had already cooled down to a temperature below that of red heat, it has happened several times, that the blow of the stamp hammer, which occasioned at the same moment, in this bar, both a local depression and a lengthening, heated it anew along two inclined lines, forming upon the sides of the piece the two diagonals of the depressed portion, and this reheating was so great that the metal along these two lines was brought back to a red hot temperature plainly enough for its form to be distinguished. These lines of greater heat

even remained luminous for several seconds, and presented the appearance of the two jambs of the letter X. In some cases we have been able to count simultaneously as many as six of these X's produced, one after another, as the bar under operation was being moved, in order to draw it out by degrees for a certain part of its length."

"The explanation of these luminous lines could not for an instant be doubtful: the lines of greater slip were, at the same time, the zones of greater developed heat." "If this fact had never been observed before, it was clearly owing to the circumstances necessary for its manifestation never having been all united together in so favorable a manner. Iridescent platinum requires for its deformation a large amount of work. Its surface is unalterable and almost translucent at a red heat; it is a medium conductor of heat: its calorific capacity very feeble; all conditions rendering the phenomenon visible in the case of forging platinum, while it has passed unnoticed in other metals."

M. Tresca afterwards was able to obtain similar results on other metals by coating the sides with wax, which was melted along these lines of slipping, when the bars were subjected to the blows of a hammer. It will be readily seen from this description that the lines thus obtained are perfectly similar in appearance to those shown upon Fig. 7.

§ 5. When a short prism of any material is subjected to a *compressive* force it *decreases* in length and *increases* in cross section until the strain is sufficient to produce rupture. In this change of form the upper and lower surfaces sustain but very slight enlargement, because they are prevented from spreading by reason of the friction at these bearing surfaces. Towards the middle of the prism the enlargement increases in such a manner as to produce a curved profile. If the prism be circular in cross section before being compressed, the cross sections will all continue circles, but of varying diameters as we approach the middle. If the original cross section be a square, the cross sections, after compression, will cease to be squares, and will become quadrilaterals with convexly curved sides. When the experiment is carried to the rupturing point, the prism is ruptured in one of two ways, either by a sliding of a portion of the prism along one or more oblique surfaces—a very common method in specimens of the metals, wood and stone—or by the prism splitting longitudinally into two or more pieces.

§ 6. When a prismatic body is subjected to a *tensile* force, it *increases* in length and *diminishes* in cross section up to the time of rupture. If the

body be perfectly homogeneous, the cross sections diminish from each end towards the middle, giving the body, longitudinally, a curved profile. If the original cross section be circular, all the cross sections will continue to be approximately circles. If the cross section was originally square, the cross sections will no longer be squares, but quadrilaterals, with concavely curved sides.

Upon rupture, the fracture will tend towards one of two forms, either square across the line of applied stress, or at an angle approximating to 45° to the line of applied stress. The latter is a very common case in steel specimens; almost perfect pyramids or cones being obtained at times. Even those specimens of steel which appear to break of squarely, show upon the edges the tendency to the angular fracture.

General Discussion.—From such observations as are given in the two preceding paragraphs, and from the familiar processes of drawing out of metals by blows of a hammer or the pressure of rolls, we see that the application of a force to a material body produces within the mass a motion, or tendency to motion, not only in the direction of the applied force, but one also at right angles to this direction.

This must necessarily be so if we assume the *volume* to be *unchangeable.* An assumption always made in hydrostatics.

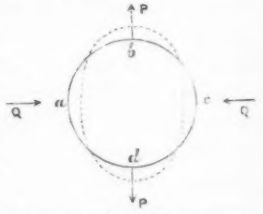
Usually it is considered that water is incompressible, but that the metals are compressible and extensible. This we believe to be a false distinction. They are all governed by the same general law—incompressible and inextensible—if by this we mean, as is considered in hydrostatics, that the volume or specific gravity is unchangeable for applied forces within the sensible limits.

This fact does not seem to have been generally recognized. The interesting results obtained by M. Tresca upon the flow of solids have now made clear the fact that the same laws govern the flow of solid bodies as the flow of liquids, the difference being one of degree only, due to the difference in the molecular forces to be overcome. Rankine defines "solids as bodies which tend to preserve a definite *size* and *shape.*" "Liquids those which tend to preserve a definite *size* only."

A *perfect* liquid, then, cannot be subjected to a *tensile* force on account of the want of molecular resistance to a change of form. It can only be subjected to a *compressive* force by resisting its change of form; for which purpose an equal resisting force must be applied in all directions;

whence follows the well known hydrostatic law of equal pressures in all directions.

Accepting the above definition of a solid, a *perfect* solid should be a body totally unchangeable in size or shape under any applied force. We know no such material. All known solids do change form under the smallest applied force, our ability to determine the same being only limited by the delicacy of our instruments of measurement.



§ 7. Any solid molecule, *a b c d*, subjected to a tensile stress, *P P*, elongates in the direction of the tensile force and contracts in the directions at right angles to the same, until the molecule assumes an oblong form, in which the applied forces and the molecular resistances are in equilibrium. Motion has taken place among the atoms in three directions—an extension along the line of the applied forces and a compression in two directions at right angles to these applied forces (one being perpendicular to the plane of the paper in above figure).

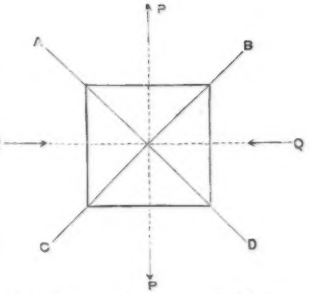
We can represent all the forces brought into action by three balanced resultant forces, *PP*, *QQ* and *RR*, at right angles to each other.

Similarly, for a compressive strain, we can consider *QQ* as the applied forces and *PP* and *RR* as the developed forces. To avoid filling this paper with mathematical demonstrations, the results of such investigations will be abbreviated from Rankine's Applied Mechanics, (referring those who desire to investigate the subject to Articles 111 and 112).

"A pull and a thrust of equal intensity on a pair of planes at right angles to each other are equivalent to a pair of shearing stresses of the same intensity on a pair of planes at right angles to each other, and making angles of 45° with the first pair."

If the pull and thrust be of unequal intensities, as is probably the case in most solids, the planes on which the shear is most intense, will still be the planes which make angles of 45°, with the axes of stress. The intensity, however, will now be an arithmetical mean between the numerical values of the principal stresses, or, in other words, one-half the algebraic difference of the stresses (giving opposite signs to stresses of opposite characters).

If P be the *intensity* of the pull, and Q the *intensity* of the thrust, the greatest shear will be upon the planes AD and BC, and its *intensity* will be $\frac{Q-P}{2}$ (P being a negative quantity).



§ 8. The lines of *greatest shear* or *sliding* should therefore be alonglines at 45° to the line of applied force, as it is in Figs. 7 and 8.

In case of rupture by compression, this does not always hold true. For instance in cast-iron, the angle according to Mr. Hodgkinson, ranges from 42° to 32° for different qualities of cast-iron. Here, however, an additional resistance. not molecular, is to be considered, viz., the frictional resistance to sliding upon the plane of rupture, due to the normal pressure upon this plane.

Other causes, such as want of homogenity, unequal resistances to stresses of opposite kinds or to the same kind of stress in different directions. or slight variations from our assumption of invariability of volume may cause a failure of the theoretical expectation.

A homogeneous material, such as the several kinds of steel, upon which these tests were made, should under a tensile strain show the theoretical lines nearly perfect.

§ 9. *What then are these scale markings?*

If the preceding reasoning is correct, they are the *lines* of *greatest shear;* lines along which the metal slides to restore an equilibrium between the applied and resisting forces or *lines of flow.*

By a consideration of this tendency to flow or shear along diagonal lines, we can understand the changes produced in solid bodies by the action of applied forces ; whether such forces be applied by means of a testing machine, hammers, rolls or loadings upon our built structures.

From the investigations of M. Tresca, we are justified in classing all tangible bodies, as more or less liable to flow—the extremes being a perfect liquid and a perfect solid.

The element, *time* must be considered; for under a rapid application of force, the most ductile body may not have time to flow, and hence would relatively be less ductile than under a less rapid strain.

The form of rupture may also be explained by the same considerations.

§ 10. The influence of a groove in a soft or ductile metal has been noticed by all experimenters.

The breaking strain (measured on the section at the bottom of groove) being always higher than for prismatic specimens. On a hard non-ductile metal, there is no such increase.

By an inspection of the lines on Fig. 9, we see why this should be; the shear lines or lines along which the metal must flow, are much longer than they would be upon a prismatic specimen of the same cross section (see Fig. 7). A non-ductile metal, having no tendency to flow, would, of course, not receive this benefit.

The above applies only to statical strains or such as are usually applied in our testing machines.

In practical construction we seldom have purely statical strains; the strains are usually more or less suddenly applied.

For such suddenly applied strains the groove is an injury instead of a benefit; here, from the want of time for the principle of flow to come into action, the strain is all concentrated upon the least section and the ductility of the whole bar lost.

§ 11. If these principles be accepted (the writer feels that the data are too few to be too positive), can we not plot the lines of transferred stress from these markings by drawing a resultant line at 45° to the several lines of shear. Then to obtain a proper form, or a form of equal strength at all parts, cannot the transverse dimensions be so taken at all points that the shear will be uniform throughout.

The time which the writer is able to devote to a full investigation of this subject, being very limited, owing to his professional duties; and his desire to interest others in the investigation and discussion of the same must be his apology for presenting the subject in so crude and undigested a form.

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CLXV.

(Vol. VII.—July, 1878.)

CUSHIONING THE RECIPROCATING PARTS OF STEAM ENGINES.

By JOHN W. HILL, M. E., Member of the Society.

This topic has already passed through a wide range of discussion by steam engineers and steam engine builders, and any effort to harmonize views variously entertained would probably be futile. It is, however, only of late years that the matter has been presented as a problem, many engineers and engine builders at the present time ignoring formulæ entirely, and resorting to the direct method of adjusting the cushion by experiment, that is, as in slide valve engines, the inside (exhaust) lap and advance of eccentric are varied until the motion of the engine satisfies the requirements of a smooth action. In setting the valves of engines similar to the Corliss, the method usually employed is to give the eccentric such an advance as will produce the desired lead, and then work up the action of the exhaust valves by the wrist plate rods until a smooth motion of engine is obtained.

Nearly all formulæ upon the subject of "cushion" presuppose conditions seldom found in practice, thus :

Absolute tightness of piston and valves.

Uniform counter pressure.

Uniform direct pressure, or pressure on steam side of piston whilst cushion is in process.

Uniformity of crank motion.

In addition to above, no note is made of the reduction of pressure in unjacketed cylinders by transfer of heat to the walls of cylinder.

All formulæ, so far as the writer is aware (excepting the elaborate demonstration of Charles T. Porter), deal with the motion of the piston as though it were uniform for similar positions in both half strokes. This, however, is an error, as the angularity of connecting rod causes the piston to approach and leave the inboard centre at the highest velocity.

The first objection noted is usually true of all new engines, and probably few, if any, operating engines are mathematically tight in piston and valve joints. If leaks exist, however trifling, corrections must be made in the adjustment of valve motion to compensate for the losses.

The lack of uniformity in counter pressure, even under the most favorable conditions, *i. e.*, uniform load and nearly uniform boiler pressure, as exhibited in the trials of automatic engines at the Cincinnati Industrial Exposition of 1875, is sufficient proof that a variable pressure must exist in the cylinder, when the load on the engine and pressure in the boiler are constantly fluctuating.

The lack of uniformity in pressure on steam side of piston at time exhaust closes is obvious, as the slightest variation in the pressure in the chest, or load on the engine, affects the pressure at every point in the stroke of piston.

The lack of uniformity in crank motion will be apparent when the action of regulator is considered. So long as the angular motion of crank is uniform, no change occurs in the motion of regulator. In the operation of the engine, however, at every half revolution of the crank the regulator revolves at several different speeds, hence uniformity of angular motion of crank does not obtain.

The loss of pressure by transfer of heat from the steam confined in front of the piston to the metal of the cylinder and to the liquefaction in the cylinder must occur when the temperature of the steam is augmented above the temperature of the metal and water.

Professor Rankine, in discussing the problem, suggests: "The most

advantageous adjustment of cushion takes place when the quantity of steam confined or cushioned is just sufficient to fill the clearance at the initial pressure."*

Mr. F. W. Bacon suggests: "The cushion should vary between one-half and the whole initial pressure, according to the speed of piston and action of valves."† What the maximum cushion pressure has to do with the *action* of valves is not clear. Perhaps the author means the action of valves determines the "grade" of cushion, which is true.

Mr. Charles T. Porter, in his admirable treatise on the Richards' Indicator, "proposes to adjust the cushion upon the weight and velocity of reciprocating parts."

Quoting from the printed circular of a celebrated engine company, the builders say: "There is a great advantage also in the great regularity of speed obtained * * * * by variable cut-off and variable compression." When it is understood that a variable cut-off controlled by the regulator will govern the speed within a range of *one-half* to *two* per cent., it seems singular that the builders would resort to variable compression, when it is not claimed that the regulation is thereby improved. This is one of those peculiar cases, however, where "the wise man makes a virtue of necessity." The valve used in this engine is a "balanced" single slide, the automatic regulator being attached to and revolving with the engine shaft, and the regulation is effected by advancing or retreating the eccentric through direct connections. Where variable cut-off is obtained by a single slide valve, any change in the function of "cut-off" produces a similar change in all the other functions, thus an early "cut-off" is accompanied by an early "release," and early "exhaust closure" with augmented "cushions," and *vice versa*. In this engine the peculiar arrangement of expansion gear is such as to make variable "compression" a function desirable or not. The builders, therefore, take a truly philosophical view of the situation, and enlarge on variable compression as a point of excellence. When it is considered that the weight of reciprocating parts remains the same at all speeds, that the velocity of piston is not permanently affected by changes in load or boiler pressure, that the direct pressure diminishes as the cut-off is hastened, it seems at least doubtful practice to employ "cushion" directly, in opposition to all theoretical deductions upon the subject.

* Rankine on the Steam Engine, page 420.

† Bacon's Treatise on the Indicator, page 66.

The following investigation, although presenting no novel feature, may be interesting to members of the Society having to deal with steam engines. The writer would suggest that the demonstration "with no cushion" is entirely practical, as he has proven by many experiments that engines may be so operated without affecting the regularity or ease of action. In such cases the momentum of reciprocating parts is balanced by lead alone.

The reduction of economy by no "cushion," and the reduction of capacity by "cushion" to initial pressure will be exhibited.

Evidently, with no cushion, the full capacity of cylinder is realized in the power developed, whilst a loss of heat occurs, due to filling the clearance space with steam at initial pressure, the clearance being previously filled with steam at counter pressure; under these conditions, the absolute loss is the difference between the heat represented by the weight of steam filling clearance at counter pressure, and the same term at initial pressure; and the proportional loss will be this difference divided by the total heat expended per stroke of piston, thus $\frac{a}{A+a} = X$, when A = heat expended in producing the power independent of heat lost, and a = the heat lost by reason of no cushion. The cost of the power will be represented by the total heat expended per stroke of piston, divided by the dynamic effect of engine, thus $\frac{A+a}{H} = Y$ when H = the work done.

If the exhaust closure is early enough to bring the pressure of the steam confined in front of piston, up to the initial pressure, then no loss of heat will take place in filling the clearance with steam at initial pressure, as this has been obtained solely by compression. But the capacity of cylinder has been reduced by "cushion" to a certain extent. Theoretically there is no loss of power by this process, as the energy absorbed in "cushioning" the steam is given up again when the piston begins the opposite stroke.

The absolute reduction of capacity of cylinder is represented by the power expended in producing the cushion; the proportional reduction will be represented by a fraction with the mean pressure of cushion referred to total stroke for the numerator, and the mean pressure for whole stroke estimated from base lines as the denominator; let p represent the mean pressure of cushion for whole stroke, and P mean pressure for whole stroke, estimated from base lines, thus $\frac{p}{P} = X^1$ where X^1 repre-

sents the per cent. loss of cylinder capacity by full cushion. Let H^1 represent the power developed, and A^1 the heat expended in producing the power; then the cost of the power is represented by the fraction $\frac{A^1}{H^1}=Y^1$. Assuming useful expense of heat in both cases to bear a certain ratio to the power developed, which is probably true of the same engine, operated under like condition of boiler pressure and cut-off, then Y is evidently greater than Y^1, hence the cost of the power is least with full cushion. Again, as the dynamic effect of piston, with no cushion is based upon the mean pressure estimated from base lines, and the dynamic effect with full cushion is based upon the same pressure less mean pressure of cushion, it is also evident that H is greater than H^1, hence the maximum capacity of cylinder is obtained with no cushion.

To illustrate the preceding propositions, the following facts are assumed: cylinder, 22″×44″—revolutions, 70—effective vacuum, 20.66 inches—clearance, 1.75 per cent. of piston displacement. The diagrams upon which the calculations are based represent the average performance of such an engine, and furnish as a mean the following values:

Initial pressure	77.5	pounds.
Terminal pressure	12.125	"
Total mean pressure without cushion	31.133	"
" " " with "	28.000	"
Counter "	3.75	"
H P., without cushion	184.0	
" with "	165.6	

Estimating the temperature of water in hot well at 140, and the pressure of evaporation in the boilers as 80 pounds absolute, then the thermal units furnished per pound of steam become 1 068.8.

The expense of heat under first condition has been estimated as follows,—the piston displacement per hour to release (98.125 per cent. of stroke) was 79 781.05 cubic feet; volume of clearance per hour, 1 422.84 cubic feet; weight of cubic feet of steam at terminal pressure, .0316 pounds, and steam accounted for to release per hour 2 565.04 pounds; the exhaust closes at end of stroke retaining in the cylinder (per hour), 1 422.84 cubic feet of steam at counter pressure; the weight per cubic foot at this pressure is taken at .01048, and weight of steam retained becomes 14.91 pounds; net steam consumed per hour, 2 551 pounds; steam per indicated horse power, 13.86 pounds, and thermal units accounted for per I. H. P., 14 813.56.

The expense of steam under the conditions of full "cushion" has been calculated thus: Steam accounted for to release, as in first case, 2 565.04 pounds. In this case, the exhaust closes at 36 per cent. of stroke (from end), and the volume of steam retained in cylinder, including clearance, 30 692.82 cubic feet; weight of steam confined 321.66 pounds; nett steam consumed 2 243.38 pounds, and steam per I. H. P. per hour, 13.53 pounds. Thermal units accounted for per I. H. P. per hour, 14 460.86. The per cent. of saving by full cushion appears to be in this instance, 2.38, while the reduction of cylinder capacity to obtain full cushion is nearly 10 per cent.

The full "cushion" has been established by Marriotte curve drawn (on the diagram) from the pressure at beginning of stroke, to an intersection with line of counter pressure. The usual construction of engines, however, is such as to disqualify the Marriotte curve in approximating actual results; thus whilst in this instance the theoretical loss of cylinder capacity by full cushion appears to be 10 per cent. experiments upon the engine prove the loss much greater than this. In engines of the cut-off class, where separate valves control the "steam" and "exhaust," it is possible to obtain the best results; but with slide valve engines such as are in the majority, where a single valve controls the several functions of "lead," "suppression," "release" and "cushion," it is possible to obtain full cushion pressure only at a sacrifice of 15 to 50 per cent. of cylinder capacity, the largest loss usually occurring with engines having balanced valves.

The writer is of opinion that the cushion should be adjusted to realize the highest economy with least reduction of cylinder capacity.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

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TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

DISCUSSIONS AT THE TENTH ANNUAL CONVENTION.

(*a*.) On Paper CXL.—The Determination of Stresses in the Eye-Bar Head. By DE VOLSON WOOD.

(*b*.) On Papers CXLIV and CXLIX.—Relative Quanities of Material in Bridges of different kinds, of various heights. By WILLIAM H. SEARLES.

(*c*.) On Paper CLVII.—Steam Engine Economy. By J. FOSTER FLAGG and E. D. LEAVITT, Jr.

DISCUSSION ON PAPER CXL.—The Determination of Stresses in the Eye-Bar Head.*

DE VOLSON WOOD.—What I have to say upon this problem will be of the most general character.

Some years ago I attended a Commencement Exercise, at which a celebrated Boston orator gave an address, in which he said: "Man is not placed here to do good, but to be good." The president of the institution followed, a few hours afterwards, in some remarks to the graduating class, and referring to this sentiment, said, in his peculiar tone and manner, "Gentlemen, the analysis is too refined." So I might say, in a general way, in regard to this analysis, "It is too refined." I do not mean, however, that it is the fault of the analysis; for in regard to that I will say that it is no more refined than the character of the problem demands. Professor Burr has made such thorough work with this problem that we are not disposed to take it up anew.

* Vol. VI, page 127. (May, 1877.)

In a general way, with regard to the effect of analysis upon the solution, practically, of problems of this character, a grand purpose is served in showing the limitations of the problems; that is, they show that certain superior dimensions need not be exceeded, and certain inferior limits must not be discarded.

But the analysis is too refined, because the conditions upon which it is founded are not realized. For instance, it is necessary, in the first part of this problem, if we follow the analysis of the writer, to assume that the material is perfectly elastic, and that the stress at the rear of the pin follows that law of fluid pressure which causes the pressure to be normal to the surface at all points, Now, the material with which we have to deal is very gross. We may say, in a practical way, that good iron and good steel are very refined; but the hypothesis of *perfect* elasticity may lead to results which are very far from being realized in practice. For my part I take but little interest in making an original solution of a problem of this kind, because I feel at the outset that I am obliged to make certain hypotheses in regard to the character of the stresses which may be far from the truth in practice, and that, consequently, the result which I may deduce will also be very far from being realized. Indeed I am sometimes surprised that the formulas which follow from the theories of Napier give results which we are enabled to use in practice. When I consider that materials are not perfectly elastic, that they are not homogeneous, that some of the materials to which the formulas are applied are not only practically but actually fibrous, I am sometimes surprised that the theory of elasticity, as applied to beams, gives results which we are enabled to use in practice. If the material of the eye was perfectly elastic and homogeneous, a trifling incident might change its character.

One day last winter a young gentleman from the room adjoining mine at the Institute, who assists in making experiments, came into my room and said: "Professor, look at this specimen." I said: "Well, what have you got here that is peculiar? I see that you are attempting to make some experiments upon a piece of tubular iron," "Yes; you see that it is drawn out here and is not drawn out there, do you?" I said: "Yes, I do." From that we entered into some explanations in regard to what had taken place and what might follow. It was desired to make some experiments to test the strength of tubular iron, subjected to a longitudinal pull. To make these tubes they took solid iron and

drilled a hole along the axis of the piece. When they had got about half the length of the piece, the drill broke and stuck in the piece. To get it out they jarred it by knocking around on the outside of it with a hammer. Now the result was that directly opposite where the blows of the hammer were applied, it did not appear to change its form in the least; but just either side of that place, it drew down, and finally broke on one side. Now I say that if an eye-bar were perfectly uniform, and a few blows were made with a hand hammer on the metal opposite the eye on one side only of it, the two sides would not behave alike, and the side hammered would probably be the strongest. No workman is careful to make the two sides *exactly* alike.

He does not strike the opposite sides exactly the same number of blows, nor with exactly the same strength. He does not observe whether there is exactly the same amount of material placed on each side before he begins. Therefore the condition of uniformity cannot be perfectly realized.

In a communication to Professor Burr I made the following remark, which had a practical tone, if not a practical value: "That a theorist who from the results of his analysis, obtained a particular formula, felt better satisfied with his analysis if it gave results which agree nearly with those which have been established by practice. And I believe that that is the practical result of these investigations. I was not surprised to find, as was reported by Mr. C. Shaler Smith, in these Transactions, that different kinds of iron demanded different forms of eyes for economy and strength. These experiments showed that for some kinds of iron a semi-circular head, back of the eye, was the best form; while for other kinds the depth back of the eye should be from $1\frac{1}{4}$ to $1\frac{1}{2}$ times the width across the eye, giving an elongated form to the outline of the eye.

We have noticed another important thing in regard to the distortion of this eye-bar—that, very much to the suprise of some, at the time the experiment was made, there was a heavy drawing in of the material, a little in front of the hole of the eye, producing a perceptible contraction in the width, notwithstanding the fact that the original width at that point was $1\frac{1}{4}$ to $1\frac{1}{2}$ times the width of the main part of the bar. This part of the bar is not analyzed in Professor Burr's paper. He says it may be rounded off by an easy curve, etc. But it might be analyzed by making certain hypothesis in regard to the shearing stress.

Another feature of this class of problems may be mentioned. A cer-

tain kind of stress takes place at the rear of the eye, say a hoop tension; another kind opposite the eye which may be partly bending and partly shearing; another in front of the eye, which may be a shearing stress drawing the material in radially, or more strictly, transversely. It is impossible to so frame our analysis that we shall glide from one condition into the other. In order to analyze any portion of it, a certain condition must be assumed to exist throughout that portion; then if an adjoining portion is subjected to different conditions, the curves which bound the two must join when the two portions meet, if the problem is correctly solved. This is one of the theoretical difficulties which arise in problems of this kind.

DISCUSSION ON PAPERS CXLIV AND CXLIX.—Relative Quantities of Material in Bridges of different kinds, of various heights.*

WILLIAM H. SEARLES.—I had thought of adding something to the analytical considerations of these papers, or of presenting the same thoughts in an analytical rather than a graphical form, as the author has given them. But time has prevented my preparing anything of this kind, and I have only a single remark to make, which may be of interest to the Convention. I had occasion last year to put this problem to practical test. The bridges of the Erie Canal were originally designed mostly by our old and respected member, Squire Whipple, and generally with a very small height of truss. They are wooden bridges with parallel chords, and without counter bracing, unless sometimes in the middle panels. They wear out so rapidly, and especially in the lower chord, from dry rot and decay, due to the weather, that it was found profitable to put in iron lower chords, rebuilding the rest of the bridge in wood. But I soon discovered that the low truss required an excessive amount of iron in proportion to the rest of the bridge, and I took the liberty of raising the truss from a height of seven or eight feet, in a span of eighty or ninety, to sixteen feet, simply to reduce the cost of the iron chord—although, of course, there was some expense in additional wood used, and in the additional length of vertical rods. When this paper of Mr. Emery's appeared I subjected the case, with the given data, to this investigation, and found that the figures gave about sixteen feet as the most economical proportion, at the then prices of wood and iron,

* Vol. VI, page 235 (August, 1877), and page 277 (October, 1877).

respectively, and of the labor upon them. I then compared the cost of the sixteen feet truss, with the eight or nine feet truss, such as we had used, and found that, in a small bridge of a single roadway there was a saving of about one hundred dollars. The height of the bridge was objected to at first by the authorities in Albany, as apparently involving extra parts, and, as it was supposed, increased expense; but I was able to lay the figures before them in such a way as to make it perfectly clear that there was a great saving in raising an eighty or even ninety foot truss to the height of sixteen feet. Of course, it necessitated the counter bracing of every panel—not to resist strain, but simply to stiffen the long braces by giving them a central support, and a system of bracing overhead became necessary, which was entirely omitted in the low trusses. The bridge, as a whole, was very satisfactory, not only to the engineers who considered it, but to the people generally, and I was assured that there were no bridges which were crossed with a greater sense of safety than those high trussed bridges, which agreed very closely with the proportions given in Mr. Emery's papers.

Discussion on Paper CLVII.—Steam Engine Economy.*

J. Foster Flagg.—It appears to me that Mr. Emery has committed the same mistake in his formula for the value of D (Vol. VII, p. 61).

$D = \frac{1\,000\,000\ H}{T - t + .0013\ H}$, that I made in my formula for the efficiency of vacuum pumps, given in my paper on that subject,† which mistake I mentioned and corrected at the New Orleans Convention—viz., including in the denominator the term $\frac{H}{772}$ or $.0013\ H$.

This term indicates the amount of heat in the steam passing through the engine which is supposed to be consumed in doing the work; if the heat which is converted into work, were abstracted from the steam in the pump, this term would be very properly inserted in the formula, but, as is very clearly explained in Porter's "Treatise on the Richards Steam Engine Indicator,"‡ in vacuum pumps, or in any steam engine using steam for the full stroke without expansion, the work is done *in the boiler*

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* Vol. VII, page 60 (March, 1878).

† Trans. Am. Soc. of Civ. Eng., Vol. V, p. 381.

‡ 3d Edition, Part four, Section one

by giving to the steam a greater number of thermal units. Even in engines whith cut off steam at some fraction of their stroke, a portion of the work is still done in the boiler, although with a very short cut-off, nearly all would be obtained at the expense of the heat in the steam enclosed in the cylinder ; so that this term would need modification for each engine working steam expansively, according to the degree of expansion.

This term is very small with the rest of the denominator, and but little error occurs from its insertion, or if correctly there, would be caused by its omission. Still, as a matter of theory, it seems to me that the formula to be correct should have this term stricken out entirely for non-expansion engines.

E. D. Leavitt, Jr.—I will not, at this late hour, detain the Convention by extended remarks on what is to me an interesting subject.

Mr. Emery's proposed basis of comparison commends itself by its simplicity and convenience.

In steam trials as usually conducted, on the basis of coal consumed, the quality of the fuel is an element of overshadowing importance, and it is possible for a comparitively inferior engine, if provided with a first-class boiler and the best of coal, to show a high duty.

By adopting the unit of heat as the basis, the engine is tested upon its own merits as an instrument for converting heat into work, and the percentage thus utilized can be readily determined, irrespective of the quality of fuel or efficiency of the boiler (as an absorber of heat).

On the coal basis, long trials are necessary because it is impossible to accurately estimate the relative value of the fires at starting and ending—the more widely these points are separated the smaller will be the error.

By weighing the feed water, noting pressures and temperatures at frequent intervals, maintaining the water level in boilers constant, and taking indicator cards near together, we have the elements for accurately determining the efficiency of an engine in a comparatively short run. A series of such trials, which can be made during the ordinary daily performance, is to my mind much better than the usual trials of forty-eight hours.

I hardly think it possible to test pumping engines accurately by the thermometric method proposed by Mr. Emery. Even if a special stirring apparatus were provided for mixing the water in the well with the hot well discharge, the mass would hardly have a uniform temperature for want

of time, and my faith in thermometers is very weak when it comes to fine measurements.

The dynamic head, by a mercury gauge at the base of the main, and the water actually discharged by the pumps, as determined by wier or other correct measurement, should be taken in computing the pumping engine duty.

Mr. Emery justly remarks that the means for obtaining economy in steam engines are well understood. This is a fact of long standing, and yet "one hundred millions" remains a very difficult achievement. Excellence of design must be supplemented by the highest practical skill and most conscientious fidelity in construction, if we are to make the last ten millions of the hundred sure.

The relative merits of compound and long stroke single cylinder engines are decidedly in favor of the compound if the same stroke, and type of valve gear be adopted. With a somewhat extensive experience in the construction and use of high speed of piston, high pressure steam and large measures of expansion (in single cylinders), I have found that as economical results were obtained when cutting off at one-fourth or one-fifth stroke as earlier; in fact, even with jacketed cylinders of small diameter and long stroke, the advantage was in favor of the later cut-off. Eighteen pounds of feed water per horse power per hour, is the best result that has come under my observation with this type of engine and in that instance there was some doubt as to the accuracy of measurement.

At the trial of the Corliss engines at the Merrimack Mills, Lowell, in 1877, the average results for one week were $19\frac{35}{100}$ lbs. feed water per horse power per hour, and I think it will be found difficult to exceed this economy in practice.

Mr. Emery's table shows that the compound engine of the "Rush," with the disadvantage of short stroke and ordinary valve gear used 18.384 pounds, and the Lawrence pumping engine but 14.019 pounds per indicated horse power per hour, which is 27½ per cent. less than the Merimack Mills engine above referred to.

There is unfortunately a dearth of reliable experiments with the long stroke marine engines that have been cited as equalling the compounds in efficiency. In a recent number of "Engineering," the statement is made that some single cylinder Corliss Pumping Engines at the water works of St. Maud, near Paris, have run with $12\frac{1}{10}$ lbs. of feed water per horse power per hour. Until we have the data for computation in detail,

such statements will not be accepted as correct by engineers in this country.

In closing, I desire to call attention to the basis of comparison adopted by Prof. Cottrill in his admirable work, entitled, "The Steam Engine considered as a heat engine," viz., "Units of heat per horse power per minute," an elegant expression involving but three figures in the statement of performance of any engine exhibiting an efficiency of .05 and upwards.

ERRATA.

Paper CLXII, Vol. vii., June, 1878.

Corthell on South Pass Jetties.

Page 135, fifth line from bottom, change word *Northward* to read *Westward.*